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## THE EARLY SURROUNDINGS OF LIFE[1]

THE American Association in its Plattsburg meeting is close to the shore lines of the first ocean that seems to have contained organic life in variety, or rather a life that had such hard parts that a tolerably complete record of the main groups and families has come down to us. It is then natural to consider what the conditions may have been under which this so varied and complex life had developed, without leaving more trace of its existence.

Reading over Darwin's "Origin of Species," one can readily see that of all the objections to his theory which he so fully and fairly presented, that which he deemed the most serious was the lack of connecting links in the geological record, and in particular the sudden appearance of the varied primordial life.

He conceded that this latter objection was valid so far as one then knew, and ventured only to suggest that while the continents and oceans had been in grand outline fairly permanent since early Paleozoic, during longer eons previous, which he felt must have elapsed, conditions might have been reversed, and the sediments then laid down have been buried beneath the oceans or altered with their life beyond recognition.

At about the same time that Darwin

[1] Vice-presidential address before Section E of the American Association for the Advancement of Science, also complimentary to the Catholic Summer School at the Champlain Assembly near Plattsburg, N. Y.

was pondering his theory, Logan and the New York geologists were studying and giving names to strata antedating the primordial animals and in the fifty years since not only has the theory of the creation of species by generation advocated by Darwin won practically universal scientific acceptance, but many thousand feet of rocks laid down before the Paleozoic have been much studied.[2] One might infer that discoveries in these beds had removed the difficulties. This is not true. The Darwinian theory has won acceptance by its marshaling of facts in other lines. The most serious difficulty still remains most serious. The years have indeed brought so many connecting links to light since Paleozoic time that we may reasonably expect to find more, and in view of the imperfections in our knowledge of the geological record and the fact emphasized by Rice last winter that the record itself is likely to be particularly imperfect just at the critical and exciting parts of the story of life, the lack of more such links seems no longer very serious.

On the other hand, the difficulty at the beginning has in some ways increased. Beds before the primordial, but little altered, such as later preserve ample traces of life—black slates, limestones, dolomites—show only obscure traces.

Nor does the total thickness of such beds suggest a time before the Paleozoic longer than that since. Astronomers and physicists are putting limits to the age of the earth, which though ample for the deposition of all known sediments, curtail the age of the planet as an abode for life to a number of years which the Paleozoic and later beds may easily have taken to form. At the same time the discovery of fishes in the Ordovician shows that the tree of Life had developed all its main branches at that early date. Where then and how did early life manage to do this "90 per cent." of its differentiation so quickly or with so little trace of itself?

Besides the answer suggested by Darwin which seems no longer admissible—it was proposed by him with the greatest reserve—three notable suggestions have been made toward lessening the difficulty. I refer to those of Brooks, Chamberlin and Daly.

Brooks imagines that the early forms of life were free-swimming surface forms of the deep sea, not freely preserved until they discovered the shore as a habitat.

Chamberlin suggests, on the contrary, that the early life was developed in fresh water, in streams and landlocked waters. Dwellers in such locations have rarely left any trace of themselves, in the rocks. Fresh water dissolves their shells, while the deposits themselves are liable to frequent rehandling.

Daly has recently suggested that the chemical character of the water was the determining factor, and that (during Eozoic time) the ocean was limeless so that the animal could not secrete hard parts.

I will not just here go into any elaborate discussion of these theories and the arguments for and against. They are not compatible. Each has almost obvious difficulties, and it is clear that farther light will be very welcome. I wish first to call attention to a ray of light which the geologist might easily overlook, since it is due to a physiologist, one of the assistants at the Collège de France, R. Quinton. He has written a brilliant book of some 500 pages[3] to defend the thesis which he had proposed in 1897 that the higher animals show traces in the vital fluid of their original environment. Let me explain.

[2] The latest fruit being the report of the Adirondack Committee, *Jour. Geol.*, 1907, pp. 191–217.

[3] "L'Eau de Mer Milieu Organique," Masson, Paris, 1904.

For Quinton the higher animals are compound colonies of individual protoplasmic cells, made up: first, of these cells, that is, living matter or protoplasm, red blood corpuscles, phagocytes, etc.; secondly, of secreted dead matter such as coral, bone matter, muscle fiber, etc.; thirdly, of various excretions, and secretions like milk; fourthly, of the vital fluid, the blood serum, free from all corpuscles or extraneous matter, the lymph, the plasm, or "physiological salt solution" which fills the body and bathes the protoplasm, and is the universal circulating fluid.

Now Quinton states that this vital fluid, which is but the water in which they live in the lower sea animals, represents the same thing in the osmotically closed higher animals, and tends to represent the original ocean or, as he formulates it (p. 417), "Animal life, which appeared in the state of a cell under definite physical and chemical conditions tends to maintain through the evolutionary series, in spite of cosmic variations, these conditions of its origin."

Quinton accordingly compares man to a marine aquarium filled, however, not with present-day sea water, but with that of the early ocean. Again he compares him to the culture tube of a bacteriologist—the tube represented by the dead matter, the skin, etc., the culture by the living cell matter, while the vital medium represents the nutrient fluid. These are striking and stimulating comparisons. It is rather a commonplace to say that society is an organism. It is not so common to say the converse: "An organism is a society."

The great value of a scientific theory is in its capacity to marshal and correlate facts, and stimulate lines of investigation. Judging Quinton's hypothesis by this test, I feel safe in classing it as a very valuable addition to science. Quinton's premise that animal life had a marine origin will, I believe, be so readily accepted by geologists that I shall take it for granted and not even sketch the elaborate arguments, embryologic, phylogenetic, and others by which he proves it.

This granted, that there should be some tendency among animals with a closed body cavity like the land animals to retain the ancestral composition of the vital fluid seems reasonable. But Quinton goes on to maintain that it is not a mere tendency, but to a very great degree successful, and that from the composition of the vital fluid one may safely infer that the early ocean had a temperature of about 44° C. (111° F.) a concentration of about seven to eight parts per thousand, and a composition mainly of sodium chloride. Now in this we can not follow him without further consideration, and we are going to ask: first, what actions are now going on which may have changed the ocean from the physical and chemical conditions of the vital fluid to those that it now has, and, secondly, what traces are there of the burial of any such waters? For there are alternatives to Quinton's hypothesis which he hardly seems to fully realize.

That environment produces on the vital fluid some effect is beyond question, and is granted by Quinton, as it is shown by his experiments. Now if by any particular modification conditions more favorable to cell life are produced, why should not this modification be accumulated by the survival of the most vigorous, and so there be a progress from the original oceanic fluid to one determined, not by the original ocean composition, but simply by physical and chemical factors, which make it the best for cell life?

Again, why may not the sea animals have remained open so long as the ocean was growing more favorable in condition for life, and only closed themselves in or struck

out for the land when in its change it passed the optimum. We shall, I think, find that this latter hypothesis is the more likely to be true. But it does not seem that we can readily dismiss the second from consideration.

There are other weaknesses in Quinton's theory, one of which we may now mention, leaving the rest until later. That is that his theory does not take in plants, and their circulating medium.

The lower plants and animals are not far apart, and we can hardly suppose a different origin. Yet plants must have existed before animals, just as herbivora must have existed before carnivora, and the cat presupposes the existence of the mouse. Plants as well as animals have protoplasm or living matter, dead matter, and secretions and a vital fluid or sap. But the sap is of much lower concentration and different composition from animals. Quinton's hypothesis needs to be supplemented by some explanation of the relations between plants and animals.

We are then prepared to consider Quinton's valuable law with an open mind, and see how far its inferences as to the surroundings of early life agree with those of dynamic and historical geology.

1. Quinton infers that the early ocean had a temperature not far from 44° C. (111° F.) which is not far from the hottest blood temperature of birds. It is certainly a remarkable fact that from the tropics to the poles, in spite of the tendency of the environment in arctic and temperate climes, the blood heat of the more vigorous and active animals in the various orders of vertebrates falls not over 10° below this temperature.

Quinton's explanation is not the only one, however. The other is that the processes of oxidation and combustion, which furnish the energy for the bodily activity, and the supply of heat to keep the body warm, raise the same to a heat which is best for cell activity, or perhaps even to the temperature which can be stood without serious damage.

A careful investigation of blood temperatures and bodily activity, which are in some ways correlated, might be very significant.

Geologically we can not speak with as much assurance as we might have before radium and the newer theories of cosmogony had undermined all certainties. Glacial periods indeed are reported upon strong evidence from early geological times. Nevertheless, whether we believe in a gradually refrigerating climate, on a cooling globe, warmed by a dying sun, or not, the former wide extent of corals and ferns toward the north pole is undoubted. Van't Hoff finds a hot climate indicated by the Stassfurt salt deposits. We must therefore allow the possibility, and I think most of us would say probability, of a much warmer ocean at least at times in the past, than at present. We may then imagine, and it was to me an illuminating thought, that the early "fish" were not cold-blooded animals at all, but active warm-blooded creatures whose blood temperature was that of the warm ocean around, which has been retained by the higher of their descendants. It seems probable that such a warmer ocean would accelerate all organic activity, including evolution.

Upon the basis of a cooling environment Quinton builds an ingenious genealogical tree. He imagines the secular cooling of the surroundings as depressing also the body temperature of all except a few forms which make special modifications to keep it up. Then a farther fall will lower all except a fraction of the first fraction, who have assumed such farther modification as

to resist this farther fall. Thus a high body temperature is a sign of a geologically recent and highly specialized form, while a low body temperature is a sign of a primitive form.

There may be a valuable suggestion here. According to it the keeled birds are more recent than the ostrich tribe, while the carnivora and ruminants belong to families more recent and more specialized than that to which man belongs. Man adapts himself to a cold climate by means of his clothes, and by use of his brains. The marsupials seem to have failed to resist the secular refrigeration at a still earlier date, while the vast bulk of the animal kingdom has submitted to the change in its surroundings and become colder and colder blooded, and all life activities slower as time has gone on. The bearing of this upon the relative rapidity of early evolution is obvious. The rate of evolution in the present cold-blooded forms may be vastly slower than in their hotter-blooded ancestors.

But suppose it to be true that there has been a cosmic refrigeration, why should life have waited for its appearance until 44° C. Many forms thrive at higher temperatures, and algæ and low forms occur and thrive in hot springs up to the temperature of pasteurization (75° C.).

If 44° C. be the temperature at which the line of descent of the birds left the ocean and it has fallen since, there seems to have been no reason why it may not have been falling before, carrying all life with it. Indeed, why should it not, if we assume that 75° C. is but barely endurable, while somewhere about 44° is the best for cell life?

The suggestion seems altogether natural and reasonable that both animal and plant life originated at a temperature above 44°, but that they followed the drop of the ocean or cosmic temperature so long as thereby better and more grateful conditions were secured.

We may remark in passing that the drop from 74° to 44° would not be likely to take as long, perhaps not half as long, as the drop from 44° to 14°, for after a body has once fairly started cooling it cools more and more slowly, the lower the temperature.

2. Again, Quinton infers that the early ocean had a concentration of between 7 and 8 parts per thousand of salts. This is the concentration of the blood serum of the birds, which seem to have kept the temperature most nearly constant, and may be supposed to have most nearly the original concentration also? But beyond this analogy this original concentration of about 7 per thousand is supported by a series of striking and important facts which seem to me to form the strongest of all the arguments, and the convincing one, indeed, for a basis of truth for Quinton's law.

The surroundings of fresh-water fishes would tend to lower the concentration. They have indeed somewhat lower concentrations, between 6 and 7 parts per thousand, but not over 8. Salt-water fishes, on the other hand, have a greater concentration. Quinton cites no figure less than 9.3, but they are always less than the present ocean (35). How can we explain the divergence of the two series from a point otherwise than that the fishes, fresh-water and salt, have been derived from ancestors whose concentration was where the one series ends and the other begins, and that the sharks have had their blood grow gradually more saline while the freshwater fishes have suffered a slight dilution of blood. But this is not all. The concentration is in man just about 8 parts per thousand, while in cattle, whose craving for salt is well known, and whose foods naturally lack sodium, so that they are very likely kept a

little short, it sinks to about 7. On the other hand, the dolphin, a relatively recent denizen of the deep, has had his concentration raised to 8.5. There are other facts given by Quinton, regarding fresh and salt-water turtles and crayfish, which I can not give.

But a host of further questions are raised regarding the blood of seals and whales and salmon that breed in fresh-water, and eels that live in fresh water and breed in salt.

We see that this implies that the ocean has grown in concentration. Quinton suggests, as it seems to me rather wildly, that the ocean is losing water into space. Putting this aside, however, we find good reason, in processes now known to go on, to believe that the ocean is accumulating salts and growing more concentrated. The water evaporated from the ocean, carried up into the clouds and rained down again upon the earth, is subject to a natural distillation. It is soft and fresh. On the other hand, the rivers come into the ocean laden with the products of solution. Murray and Dubois have given valuable tables of river composition. The result is that every lake without an outlet, receiving a river, like the Great Salt Lake or the Dead Sea, becomes very salt. And what is the ocean itself but a much vaster lake. So Hunt, Joly, Dubois, Macallum and most of those who have given the subject any especial thought have inferred a concentration of the more soluble salts in the sea. We can not, however, separate a full discussion of concentration from that of the composition of the ocean. I can not see any escape from the general conclusion that there must have been some such concentration, except by supposing that volcanoes from absorbed gases in the interior of the earth are yielding more water than enough to counterbalance the soluble salts brought in by the rivers. This we can hardly disprove, if we assume that there may also be a gradual transudation of these waters from the interior, but it does not seem likely. While, however, Quinton's theory agrees with processes now going on in its suggestion that salts have accumulated in the ocean, we are impelled to ask, as we did in regard to the temperature, why should life have waited to begin until the ocean was already brackish, or why should the ocean have begun with a concentration of 7 parts per thousand?

May it not be that life began in an ocean of much less concentration, but remained open to it, with the body cavity not cut off, until that best for cell life, to wit, a concentration of nearly 7 parts per thousand, was reached?

If we ask this we are led to turn to historical geology, and to the line of enquiry which is, I fear, the most difficult, the least certain, and the most tedious, but to which I feel bound to devote some time, since it happens to be the gateway which led to my interest in the whole subject.

We may ask, what indications are there of any such concentration less than the present in the waters buried in the earlier strata?

Do they come at all, and if so, at the beginning of life, or at that stage in the geological column when the land animals and those who may with some assurance be supposed to have a vital fluid distinct from the sea water are known to have existed? But before we can apply this test we must ask and answer many difficult questions. Can we find analyses of rock waters which can be fairly assumed to represent buried ocean waters? Has not the circulation in strata in the course of millions of years been thorough? Then, again, how can we tell, even supposing that there has been no such circulation that strata were not laid down and filled in the beginning with more

or less fresh water? For freshwater springs, as Hitchcock and others have described them, are not uncommon beneath the ocean, and very commonly fresh water may be found by digging in the sand of ocean beaches.

Such an inquiry would have been impossible in Darwin's time. But since his time so many holes have been put down to very great depths by churn or diamond drill, in search of oil, gas, brine, artesian water, etc., that it does seem fair to ask the question. Yet we can hardly separate in the inquiry the composition from the concentration. For in the proportion of the elements, especially of the chlorine, seems to be the surest test of the character of the water. I shall not try to prove this in detail, as it depends on study of many water analyses, but I may give you some idea of the reason. Chlorine (Cl) now exists in ocean water in excess of sodium, and there is, as we shall see, good reason for believing that that excess has been even greater than now in times past.

By an excess of chlorine I mean that after chlorine has been set aside enough to combine with all the sodium that the analysis of a water shows, there is still chlorine left which is usually understood to be combined with the potassium, calcium and magnesium present.

On the other hand, in waters that are mineralized by leaching, whether from granites of New England or the alkali plains of the west, and in all rivers with very few exceptions, explicable often by contamination with manufacturing wastes, there is sodium enough to combine with Cl and then some left. There is hardly any rock, but the unique Stassfurt deposit from which one can imagine chlorides other than sodium chloride to be leached. Nor, on the other hand, is chlorine in solution easy to get out again. The rare and valuable silver ore, hornsilver, a few significant volcanic chlorides, quite soluble, and transient around active volcanoes, and the deposits of dried up oceans and lakes, and a few minerals of no quantitative importance like apatite and sodalite, complete the list of chlorine minerals.

To get results of any value, one must take all the available analyses of a region, see what the surface waters are, how they seem to be affected in composition as one follows them in depth, eliminate waters that may come from desiccated seas, gypsum beds, or salt beds, or may be erroneous and contaminated. Gradually I find myself forming some idea of what the buried water must have been. I must frankly own that from my studies in that state with which I am best acquainted, Michigan, I was first inclined to think that there was no sign of earlier, weaker ocean waters. On the contrary, deep wells from perhaps every geological horizon seem to have stronger brines than the ocean.

Upon taking a wider view I have come to change my mind, and think it is not altogether accidental that the water at Sheboygan, Wisconsin, has at a depth of 1,340 feet 10 parts per thousand of solids, of which 4.3 are Cl; that the deepest well at Cincinnati, 1,245 feet, has 11 parts per thousand, of which 6 are Cl; that Litton's very careful analysis of the water from a well 2,200 feet deep at St. Louis (the water mainly at 1,515 feet in the Magnesian limestone) should give 8.791 per thousand, of which 4.1 are chlorine, and that near us at Montreal in rocks of similar age in a 1,500-foot well (at 1,190 feet) Adams found 7.57 per thousand, of which 2.46 are chlorine. In Europe the only well at all similar I have yet found is one reported by Struve at St. Petersburg (658 feet through Silurian rocks to the granite) with 3.89 per thousand, of which, however, 2.3 are chlo-

rine. Now compare these with the 35 parts per thousand, of which 20 are chlorine, of the present ocean. Consider that in each case this is the saltest water from the district and horizon. Do they not indicate for the open paleozoic ocean a concentration much less than at present, and not far from that of the vital fluid of the land animals, at the very time the first land animals are known to have appeared? The large collection of water analyses made by Blatchley for Indiana yields exceptionally good material for study, since there are a large number from drilled wells, in many cases separated from the surface by sheets of oil or gas. We have to eliminate only two or three whose extra strength appears to be due to brines of the Salina formation, or to solution of sulphides as sulphates on the one hand, and more numerous shallow wells, where the water is dilute, and carbonates, not chlorides, dominant on the other, to get a well-defined and extra abundant group of analyses, characterized by a chlorine excess, and a concentration of 500 to 1,100 grains per gallon, *i. e.*, 7 to 16 parts per thousand, all drawing their strength deep down in Paleozoic rocks.

Norton's collection[4] gives us a chance to study Iowa in similar fashion. Here there seems to have been much circulation from the high western plains toward the Mississippi Valley. Yet, if the rocks had been laid down in water as salt as the present ocean to any great extent, it seems to me hardly likely that that which contains the most chlorine of all those Norton gives would have but 3.356 parts per thousand, of which 1.1 only are chlorine, while there is a large excess of sodium, showing, to be sure, considerable dilution. This water is from 716 to 845 feet down in the Carboniferous.

This is a work in which it is easy to deceive one's self, and many should share in these critical studies.

[4] Vol. VI., Iowa reports.

3. We come now to the third point made by Quinton, the resemblance of the vital medium to the ocean in composition.

Sodium chloride is the leading salt in both. This is the more noteworthy because in the living cell potassium and phosphates are much more important, and this is also true in most of the animal foods.

That is the reason why the craving for salt is so natural, and the salt tax the one tax that the poorest mortal can not evade, if he would live. It is the last screw to be placed on abject poverty.

A second group of constituents in abundance is formed by magnesium, calcium, potassium and sulphur. Carbon, oxygen, nitrogen, hydrogen (carbon dioxide and ammonia), silica and fluorine are also well known to be present in both. Finally, by an elaborate discussion of physiological literature Quinton shows that all the rarer elements of sea water, with the exception of cobalt, also circulate in our veins, to wit, I, Br, Mn, Cu, Pb, Pt, Zn, Ag, As, B, Ba, Al, Sr, R, Cs, Au, Li.

I do not consider the identity in the presence of traces of relatively rare elements of any very great importance in the present state of analysis. These elements are all widely distributed in nature, even if in minute quantities, and are liable to occur in ocean or vital medium without there being any genetic connection. Moreover, the knowledge of their presence in the one case or the other depends in a number of cases on only one or two determinations, so that there is no assurance of their universal and constant presence.

When we consider the ratio of the different substances, which is what Quinton means by the composition, we find a number of difficulties as well as some striking resemblances between the vital fluid and

the ocean. One difficulty is in getting what may be fairly called the composition of the vital fluid. Ordinary analyses of the blood, for instance, show percentages of phosphates and an excess of sodium over chlorine entirely foreign to sea water, present or past. But when the blood cells and organic matter have been most carefully eliminated the proportion of phosphoric acid drops to but 25 parts per million, and though this is more than occurs in sea water now, it is very much less than occurs in the body generally, and Quinton's supposition that it is due to a small amount of organic matter that one can not get rid of, or that it comes from excretions from the organic matter which the blood is carrying off, is not unreasonable. We may even, when we look at the analysis of the Sheboygan water, and consider the phosphatic character of the early brachiopods, imagine that there was in early days more phosphorus than the present ocean contains, and that it had been eliminated by life faster than supplied.

Similar explanations might perhaps be given for the excess of sodium.

The ratio of sodium to potassium is in the vital medium 10 to 1. In the ocean it is nearer the famous 16 to 1. This is a great contrast to living matter in general, in which potassium dominates over sodium.

When we come to compare lime and magnesia, however, we find that both in living matter in general and in the ocean at present magnesium dominates (1.31 : 0.47) over calcium. On the other hand, in the vital medium there is three times as much calcium as magnesium. This fact did not escape Quinton and Macallum, and they both suggest the same explanation—that there has been an accumulation of magnesium in the ocean since it determined the composition of the vital medium.

This is chemically and geologically very likely. The magnesium salts are more soluble than the lime salts. Again deposits from the ocean of lime sulphate and lime carbonate with little or no magnesium, gypsum and limestone, are well known. On the other hand, the only common magnesium deposit known in the rocks, dolomite, has a molecule of calcium for every one of magnesium. Thus a relative increase of magnesium over lime seems probable. Dubois brings reasons to believe that the ocean is now saturated with calcium carbonate, and has as much as it will hold, and that lime is thrown out, largely in coral and shell, as fast as the rivers bring in carbon dioxide. The magnesium which the rivers bring will readily remain as sulphate or chloride and is not so easily reduced or precipitated as calcium sulphate, nor is magnesium so freely taken up and thrown out by organic life.

Thus while the problem is not a simple one, since it depends on the supply of sulphur and carbon as well as magnesium and calcium the accumulation of magnesium seems, to say the least, quite possible. In the present ocean Na : Mg : Ca : : 10.23 : 1.31 : 0.47. As regards the sulphur salts, Daly has suggested that, as in the Black Sea so in the early ocean, until the carnivorous habit was well established and a good scavenger system, the sea would tend to be fouled with dead matter and the sulphates brought down by the rivers be reduced and deposited as sulphides, as we find them in black shales and organic limestones.

No doubt this action has occurred from time to time. It agrees with the customary association of pyrite and black shales, and petroleum and sulphur. But what indications are there of greater frequency of sea-deposited sulphides in the pre-Cambrian rocks?

Anyway if the accumulation of magnesium in the ocean depends on the supply of

sulphate we must expect to find it as well as sulphate low in early times.

We are thus brought face to face with a very important problem of historic geology, the chemical evolution of the ocean.

Studying the Paleozoic waters, we find unmistakable indications that they were low in magnesium sulphate, but that they contained relatively large quantities of calcium chloride. This, as long ago noted by Hunt and Goessman, is characteristic of the Paleozoic brines. $CaCl_2$ must have been precipitated little by little by the carbonates and sulphates, forming the calcium carbonate and sulphate of the sedimentary rocks, while the sodium and magnesium remained in solution.

The ratio of calcium to magnesium in the vital fluid is about 0.10:0.025, say four to one, while in the Saginaw carboniferous brines it is usually near three to one.

Here for the third time we come to the conclusion that the vital fluid has a composition that the ocean is likely to have had in times past, not at the beginning of its progressive change, but at about the time when we find the first trace of vertebrate life.

4. For the Ottawa meeting of the Geological Society of America I prepared a paper on the chemical evolution of the ocean, and I showed a diagram in which analyses of waters from different strata were arranged according to the ratio of sodium to chlorine, which appeared to have increased in the ocean from something like .20 in Calciferous times to .555 at present. A change in concentration of from 8 to 10 parts up to 35 in the same time would be of the same order, but would imply considerable additions of chlorine in the same time, which we should have to look to the volcanoes and their rocks to furnish. It would also imply, if both changes were uniform, which does not seem likely, that when the concentration by river action began the ratio of sodium to chlorine was somewhat about 0.06. Even in the weakest water and at the beginning the ratio of sodium to chlorine must have had some value, and this is about the ratio in the deep water of the Keweenan copper mines, and may be the ratio which comes from juvenile waters, or those emitted by volcanoes, or may be that due to the leaching of volcanic rocks.

For the fourth time the suggestion is forced upon us that the vital medium does not represent the early ocean, not that which first began to cool, nor that in which the rivers first began to bear their burden of dissolved salts. It does seem quite nearly to correspond to that which we have other reasons to believe existed at the beginning of the Ordovician or end of the Cambrian, not long before the time that the first fishes are known to have existed, but much after life is known to have existed.

The evidence goes then to support Macallum's modification of Quinton's theory (conceived quite independently) that the vital medium represents the ocean water at the time the body cavity in the progress of evolution became osmotically closed.

It thus seems likely that we have found a key by which we may date the development of life and the deposition of beds in terms of the development of the ocean, whenever we can get a sample of the normal ocean water of the time, or approximate to it. I need hardly say that it is not likely that this change in the ocean proceeded at a uniform rate. Changes in climate would affect the great distilling process upon which the development rests. Great uplifts might hasten the supply of salts. Geographic conditions leading to the deposition of enormous salt beds in partially cut-off bays might reduce the con-

centration of the open ocean. As the area of sedimentary rocks increases relatively to that of igneous rocks the character of the supply of salts furnished by the rivers must change.[5] On the whole accumulation was probably more rapid relatively to the area of land surface, at early dates, when evaporation was more powerful. It is doubtful if the land was so well forest clad and protected from erosion as later. Moreover, salts would be less likely to be thrown out when the ocean was weaker and not so saturated.

The broad conclusion of a gradual accumulation of salt in the sea, particularly sodium, is confirmed by so many independent lines of evidence as to have a very small probable error.

Let us review the arguments for accumulation of sodium before leaving this part of the subject.

1. The argument from the erosion of continents developed by Mackie.

2. The argument from the excess of sodium in river water, worked up recently by Dubois after Murray and Joly.

3. The argument from the shortage of sodium in the average sedimentary rock as compared with the average igneous rocks from which it is derived, as developed by Van Hise, Clarke and Mead.

4. The argument which may be drawn from a study of the early buried waters as suggested by Hunt.

5. And finally the argument which as we have seen follows from the composition of the vital fluid and Quinton's law.

These various arguments depend on various lines of facts, so that one may be fallacious without disproving the other. On the other hand, the stronger lend strength to the weaker. For instance, I doubt if any one from mineral water analyses alone would be likely to feel much assurance. The errors and defects in the chemical work, in the collection of samples, and in the circulation and preservation of the waters, are too great.

The one thing that seems most assured regarding the buried waters is that the early ones were relatively richer in calcium chloride.

The many published, and dozens of yet unpublished, determinations upon our Michigan waters, have put that in my mind beyond question.

The analyses of the coastal plain post-Paleozoic waters deserve farther study. The saltest well of the analyses recently collected by Smith in Alabama from the Tertiary has 30.5 parts per thousand with 11.47 sodium against 18.52 Cl., while the ratio of magnesium to chlorine is .1224: .247.

This is not far from what we might expect from a buried sea water of this age, but on the other hand among the waters from the Cretaceous there is none that appears to be at all unmixed sea water. The saltest, and at the same time one of the deepest (IVa) has only 8.57 per thousand and Na:Cl::2.999:4.538 = 0.66, while Mg: Ca::.0431:.1396.

The analyses collected by Veatch in Louisiana, where we know that rock salt beds occur, show in many cases greater concentration of salt than the present ocean. They also show very little of sulphates, but they show a ratio of magnesium to calcium quite different from the Saginaw brines which mark them as relatively recent.

Leaving now the tedious numerical part of our subject, we may ask ourselves what bearing this has upon the difficulty to which we referred at the beginning.

The vital fluid seems to date or preserve the ocean composition, concentration and

[5] In early times the supply of chlorine seems to have been relatively more rapid, being derived from the leaching of the Keewatin rocks.

temperature of somewhere about Beekmantown eo-Ordovician time, somewhere about the time that the ocean had attained one fifth of its present concentration. This indicates that the pre-Paleozoic was probably not one fourth as long as later time, dating the beginning when the ocean began to get concentrated.

Thus the rapidity of early evolution, to which physical, astronomical and stratigraphic evidence had led us, is confirmed.

But we know that life existed all through Cambrian time, and earlier yet. So while Quinton's investigations indicate certain conditions of the vital circulating fluid from which fresh-water and salt-water vertebrates have diverged, while the more vigorous land animals have retained them nearly unchanged, these conditions are by no means so near the beginning as one might think from a casual reading of his argument. It seems much more probable that life began as soon as was possible, at higher temperatures than 44° C. and at very low concentrations. The water was that leached from basic rocks, the Keewatin schists, and was relatively richer in calcium chloride, and I suspect also ferrous chloride,[6] and was in composition as well as concentration by no means the best for organic life. We may then believe that as the temperature of the water decreased, and the concentration increased, this change was in the direction of the physiological optimum, or salt solution most favorable to protoplasm and cell activity.

According to Meyer's account of the stimulant effect of sodium chloride, calcium and potassium, and the sedative effect of magnesium, the early ocean, as it accumulated salts of sodium and lime, must have been a more and more stimulating medium, up to a point when it became overstimulating and poisonous. Up to this time, which I take to be about the beginning of the Cambrian, there would have been no physiological tendency to secretion or excretion of lime by animals, until and unless the ocean became supersaturated with carbonate. The ocean in the beginning must, like fresh water to-day, have acted as an active solvent if any were formed. But when it passed the optimum then the excretion or precipitation of lime and accumulation of magnesia, which is more sedative, might tend to restore the balance. So long as it could thus be kept in most favorable condition for cell life, there would be no especial reason for osmotic closing or the development of a special vital fluid. Up to this point it would pay animals to accept the beneficial changes which were taking place in the medium in which they lived and moved and had their being.

We do not, understand, suppose that the ocean arrived at the best conditions for cell life in all respects at the same time, but we suggest that one of the first endeavors of the more vigorous animals to keep the vital medium of the best was by means of secretion of superfluous lime. This is supported by the fact that lime is the substance most abundantly brought in by rivers, one in which saturation would soon be reached of the carbonate, and by the fact that lime skeletons and hard parts begin to be abundant in the Cambrian, a geologic period before those animals appeared which we may be sure had a separate vital fluid. However, our general belief in the critical importance in the history of life of the time when the ocean passed through the most favorable conditions for cell life does not depend on any particular theory of the physiological interrelation of various salts.[7]

[6] Compare the richness of iron carbonates in the Huronian. Probably mixed carbonates were precipitated as fast as carbon dioxid was furnished.

[7] But we do suggest that the secretion of hard parts began first as a physiological reaction, like renal calculi, to the increasing hardness of the vital medium.

As the ocean passed more and more decidedly this optimum, the more vigorous organisms resisted this change in various ways. They cut themselves off from it osmotically. They secreted a more or less impervious carapace or shell. Then they got out of it entirely and migrated to the air or the land, perhaps by way of the shore sands and muds. This period when the ocean seems to have passed its best stage for life appears to have been the Cambrian. After this period there was a wealth of forms able to leave hard traces of themselves. Before this period there was no physiological need for skin or shell. But once the skin and shell had been developed, primarily as a physiological reaction against the water, their great advantages for purposes of defense and support no doubt soon made themselves felt.

Before this the early dilute and perhaps acid ocean water would attack shells freely. After the Cambrian time there was an excess of calcium carbonate, which has been steadily thrown out, as the rivers brought the carbon dioxide in, ever since.

In passing I may say that Macallum has suggested that from the composition of the protoplasm itself we may form some idea of that of the early ocean, but as I can not endorse his conclusions, I will not dwell on them here.

We thus attribute the development of hard parts and a separate vital medium, the one occurring at the beginning, and the other at the end of the Cambrian, to the same cause, the endeavor of the societies of cells we call organisms to maintain for the mass of the constituent cells the best possible conditions for their activity.

We have thus, I conceive a fair explanation for the rarity of traces of hard parts in the early rocks. The animals had felt no physiological need of them and had not begun to develop them, and the ocean was relatively fresh, and would more easily dissolve them.

Moreover if the early ocean was changing in composition and growing more favorable to cell life, while the organisms were bathed in it, we might well expect a rapid evolution, and one not merely superficial.

It is a standard doctrine of biology that the individal in his growth gives a sketch of the history of the race to which he belongs, that embryology and phylogeny run parallel. I do not know that any one has heretofore suggested that the time occupied in passing from one stage to the corresponding stage is in any way proportional in the two series, and that the rapid changes in the egg at the beginning are matched in racial history. It would be indeed foolish to hold this in any rigid way, for the parallelism is in no sense strict. Yet is it not fair to suppose that life as a whole was more plastic at the start even as the individual is, and that as in artificial races there is a cumulative effect of heredity, tending to hold them true after a few generations, so it must be for organic life as a whole?

As we have said, if we may estimate by the concentration of the vital medium relative to that of the present ocean, and suppose the increase to have been uniform, the time prior to the separation of a distinct body fluid, that is, the pre-Ordovician, can be only a quarter of the time which has elapsed since. A discussion of the temperatures and change of ratios of sodium to chlorine and magnesium to calcium, in the present ocean and Paleozoic waters while they do not agree closely, also lead to the conclusion that the ocean at the beginning of concentration by erosion was only a fourth older than the early Paleozoic ocean.

This will give us room for all the sedi-

mentary column of Huronian and Laurentian, I think, for I think the pre-Ordovician column of rocks is hardly more than one fourth of that since.

I presume some will grumble at being curtailed to so short a time before the Trenton, only twenty million years or so (if the time since is eighty million years). As we have said, the progress is probably not exactly uniform. But really twenty million years is quite a while. The following illustration may help us to appreciate this. There has been a change of 15 per cent. to 20 per cent. in the flora of Michigan and Ohio in the past 200 years. If then in each year there was an average deposit of but one two-hundredth of a foot there could be an accumulation of ten thousand feet of strata with a 15 to 20 per cent. change of plants every foot in twenty million years. Have we any facts to make us feel sure that that is not time enough?

The growth of scientific doctrine and theory is like that of some modern invention like the steam engine. Different men contribute, the one this improvement, and one that, until in looking at the perfect machine one wonders and admires and forgets that it is an embodiment of the ideas not merely of one designer, Watt, or Corliss, or Nordberg, but of many, who have each contributed something. So many have discussed the physiological salt solution and the oceanic origin of life, but Bunge, Macallum, and especially Quinton have brought them into relation. Before them Hunt, Goessman, Joly, Mackie, Dubois and others have theorized on the evolution of the composition of the ocean.

From them all we have borrowed, or by them been anticipated. So of previous writers on the conditions of early life, it will be seen that I agree with Chamberlin as to the relatively fresh character of the early medium in which animals appear, that I adopt Daly's suggestion that the scarcity of hard parts of pre-Cambrian animals was physiological and due to the chemical character of the ocean, though we can not at all agree with his conclusion that it was limeless, which seems to be negatived by the composition of the vital fluid, the evidence of fossil brines, and the deposits of the early oceans.

What we bring (besides some detail studies of buried waters) is the correlation, and the suggestion that the development of hard parts, and a relatively permanent vital fluid, were *both* physiological reactions to the chemical evolution of the ocean, as it reached and passed its best conditions for life in the early Paleozoic.

The wider the area of our knowledge the greater the circumference of our ignorance, and the test of a good theory is that it opens up new lines of research. Let me mention a few. Further tests of the composition of the vital medium would be very interesting, especially in salmon, and eels and the like, as well as in seals, whales and insects.

The field which I hope to help cultivate, myself, is the study of waters which may be in part buried sea waters. It would be very interesting to extract the quarry moisture of impervious rocks. I do not know how to do it, without danger of extracting solid constituents at the same time.

To sum up in conclusion, it seems likely that early evolution was very rapid—the history of the race in this respect being like that of the individual—because of some of the following factors:

1. A warmer ocean, and consequent greater activity of life.

2. A constant approach of the same up to early Paleozoic times toward better conditions for life, which caused the organisms not to cut themselves off from it, but remain open, while hard parts were rare, thus

exposing the organisms throughout to the modifying effects of environment.

3. The relative weakness of the stereotyping effects of cumulative heredity at this early date.

4. The fact that as all available spots were not preempted, there were wide fields open to successfully modified forms adapted to some new yet unoccupied station, who could then be very prolific, and thus give large play for further adaptation.

5. The frequency of generations in the lower animals and plants.

6. Probably a relative lack of seasonal rhythm.

7. While new forms of life and the flesh-eating habit were being developed a stimulus was put on various modifications to meet these new conditions.

Since early Paleozoic times animals have existed fitted for land and sea, salt water and fresh, air and mud, herbivorous and carnivorous, with the main methods of attack and defense outlined. So that one could hardly expect so radical or rapid changes thereafter.

I think this audience in this assembly will permit an old pupil of Shaler to indulge in a little philosophy and close on the eve of Sunday with a moral.

Haec fabula docet: that those societies of cells known as animals have not been the mere slaves of environment, nor even of environment and heredity conjointly, but have struggled, with more or less success, to maintain through varying environment that part only of their heredity which conduced to greater protoplasmic activity (or, to put it in every-day English, have striven to surround the great mass of the cells of which they are made up with the conditions best for their health and vigor), and the physical grade of the animal is in the ratio of its success in this struggle for the common weal of the constituent cells.

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## SCIENTIFIC BOOKS

*Researches on the Affinities of the Elements.* By Geoffrey Martin.

The unfortunate part of the book is that any attempt to separate the grain from the chaff is made difficult by the amount of chaff.

The author attempts to represent the affinities of the elements for each other by means of a diagram. This diagram for any particular element is constructed by arranging the elements according to the periodic table. Then over each element erect a perpendicular whose length shall represent the affinity of the chosen element for the element in the table. Joining the points (ends of the perpendiculars) thus obtained, you have a surface which represents at a glance the affinity of the chosen element for all of the elements.

The fact that such a surface could be constructed by any one having a knowledge of the proper lengths of the perpendiculars needs no proof, and the author's mathematics on this subject could, we think, have been omitted with advantage. The real difficulty arises in determining the length of the perpendicular which shall represent the affinity in any given case. Where the heat of formation is large this is taken as measuring the attractive force. Where the heat of formation is small, or the data insufficient, the author determines the proper length by a comparative study of the compound with regard to its stability, etc. The surfaces as given by the author for thirty-one substances are therefore not claimed to be exactly quantitative, but only qualitative. The author points out the readily perceived fact that the form of the surface would be altered should the arrangement of the elements be changed. Also it would be altered by changes in pressure and temperature and would change its form completely with a change in the valence of the element.

We think it hardly probable, even where the heat of formation is large, that it gives any very good idea of the actual size of the attractive forces involved. For the elements reacting are usually previously not monatomic and the heat of formation is the sum of the several reactions involved, to say nothing of a variable amount due to the physical changes. Moreover, T. W. Richards and J. Traube have separately pointed out that the heat of formation is probably largely dependent upon the changes in volume suffered by the reacting elements. When these facts are all considered the affinity surfaces constructed by the author at the expense of so much labor can hardly be taken as more than a guess. Much will have to be learned before any true affinity surface can be constructed.

From various facts cited by the author as collateral evidence bearing upon his point of view some of his conclusions are worthy of note either as being new or because they are derived from a more or less novel standpoint. Thus on page 42:

> It seems, in fact, that the force exerted by the bromine atom is always less than the force exerted by the chlorine atom on a given element or radicle, but is always proportional to it.
>
> The chemical attractions which *A* exerts on the various elementary atoms or radicles must be either equal or proportional to the chemical attractions which *B* exerts on the same atoms or radicles.

On page 74:

> It is the intensity of internal atomic forces with which the atoms are attracted together in the molecule which determines the intensity of the external attractive force between molecule and molecule, and therefore the volatility of the compound.

On page 99:

> Strangely enough, the melting-points of similar compounds does not appear to depend upon the weight of the molecules, but upon their chemical nature, because, as a rule, they melt at temperatures which lie close together in spite of great differences in their molecular weights.

On page 101:

> The melting-points and solubility in water can not depend largely upon the magnitude of the molecular weight, but must depend upon the chemical forces.

On page 111:

> Consequently we infer that the volatility of compounds is determined almost entirely by the intensity of the forces which bind together the atoms in the molecule.

On page 219:

> If this be so, a non-metal is nothing more or less than a substance viewed at a temperature too low for it to assume metallic properties; and conversely, a metal is a substance viewed at a tem-

perature too high for it to assume non-metallic properties.

So that the metallic and non-metallic conditions are simply phases, which all kinds of matter pass through as the temperature increases from zero upwards.

His explanation of osmotic pressure and solubility given in Chapter III. is worthy of note.

There are numerous misprints in the book.

We would end finally by a quotation from the preface and would give warning that the author's speculations are not confined to the preface and the appendices:

> In appendix C is discussed from this point of view the habit of alcohol drinking, and it is suggested that it may be the beginning of an organic tendency that will ultimately lead to the elimination of water in living matter, and its replacement by the more mobile alcohol, in order that as the temperature of the earth and sun falls the aqueous fluid in living matter may be replaced by alcoholic fluids which will remain liquid under conditions which convert water into a solid state.
>
> It is indeed a very curious fact, which has never been adequately explained, that men seem almost instinctively to avoid the use of pure water as a beverage. They drink either tea, beer or alcoholic liquids, but only water when they are either very thirsty or when other liquids can not be obtained. There must be some scientific cause underlying this tendency, and I think that appendix C opens out a very curious possibility as to what this tendency may ultimately lead to.

J. E. Mills

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## SCIENTIFIC JOURNALS AND ARTICLES

*The Journal of Experimental Zoology*, Vol. IV., No. 2 (June, 1907), contains the following papers: "The Influences of External Factors, Chemical and Physical, on the Development of *Fundulus heteroclitus*," by Chas. R. Stockard. The eggs of *Fundulus* were found to produce definite types of embryos when treated with various salt solutions. The most striking type being the one-eyed monsters resulting from the use of sea-water solutions of $MgCl_2$. Osmotic pressures resulting from the use of sugar solution affected the eggs much more violently when they were being developed in fresh water than in sea-water. The effects of a weak salt solution are augmented by the addition of sugar to the solution. The embryos develop in a perfectly normal manner entirely out of water if kept in a moist amosphere, though they are unable to hatch unless put into water; then they very promptly break through the egg membrane and swim away. "Movement and Problem Solving in *Ophiura brevispina*," by O. C. Glaser. "*O. brevispina* moves in practically all of the ways possible for a pentaradiate animal; exhibits no sign of improvement from practise in the performance of the righting reaction or of freeing its arms of encumbrances. The behavior, in spite of its complexity, can not be considered a sign of intelligence." "Occurrence of a Sport in *Melasoma (Lina) scripta* and its Behavior in Heredity," by Isabel McCracken. "In this paper the author records the results of a breeding experiment carried through a series of seven generations, under controlled conditions of a dichromatic species of beetle in which a "sport" is of occasional occurrence. The results show that the sport, although inherently stable, as evidenced by its breeding true through selection, is entirely dominated by each of the dichromatic extremes of the species in a first cross, and is gradually eliminated from the lineage of each of these in successive crosses. "The Energy of Segmentation," by E. G. Spaulding. The paper presents the application, by means of experimental methods, and not simply as a postulate, as has heretofore been the case, of the first and second laws of thermodynamics in their generalized form to the event of segmentation. These methods were "compensation" methods; cleavage, in sea-urchin eggs, was inhibited by means of osmotic pressure, and from the values thus obtained and with volumes and surfaces known the energy-change was computed. The conclusion is reached, that, with these laws valid for the organic as well as the inorganic realm, these two realms fall as species within the same "natural classification" in which the principles stated by the two laws form the highest genus. "Experiments in Transplanting Limbs and their Bearing upon the

Problems of the Development of Nerves," by Ross Granville Harrison. When the rudiment of the limb of a tadpole is transplanted it acquires after a time nerves which are connected with the nerves of the region of implantation. The nerves have the same arrangement and distribution as those of the limb in its natural position. This is the case even in limbs taken from individuals which have undergone their development after having been deprived of their nervous system and also in the accessory limbs which sometimes bud out from the transplanted appendages. The nerves in question are not preformed in the transplanted limb but they actually grow into it, their mode of distribution being determined by the structures within the latter. The development of an embryonic nerve ceases and degeneration sets in as soon as the connection with its ganglion is severed.

## *SOCIETIES AND ACADEMIES*

### THE GEOLOGICAL SOCIETY OF WASHINGTON

The 193d meeting of the society was held on May 8, 1907, Vice-president Campbell in the chair and sixty-two members present. Under the head of informal communications Mr. F. E. Wright exhibited artificial crystals of silver, copper and diopside produced under various conditions in the Geophysical Laboratory of the Carnegie Institution of Washington and discussed briefly the bearing of the different modes of formation on the general theory of the precipitation of native copper and silver ores. Mr. Lawrence La Forge exhibited a new orthorhombic pyroxene found in a slag at Bingham, Utah. Although this mineral was found by Mr. Wirt Tassin, of the National Museum, to have the chemical composition of a normal calcium iron pyroxene, crystallographically it was found by Mr. La Forge to be orthorhombic but with the same prism angle within the limits of error as ordinary monoclinic pyroxene. Six different crystal forms in all were observed, the prevailing habit being that of an elongated square prism, termination either by the base or by an oscillatory combination of the base and a bracydome. An attempt was made to determine the optical constants but was unsuccessful because of the dark color of the mineral and shattered condition of even very small crystals. Two sections, however, were ground thin enough to determine the extinction, which was found to be parallel and the orthorhombic character thus confirmed.

#### *Regular Program*

*The New Map of the Yosemite Valley:* F. E. Matthes.

This topographic sheet of the U. S. Geological Survey, about to be published, affords a particularly instructive example of modern detail mapping, in that it suggests possible criteria for the guidance of the topographer in the construction of maps which shall embody a scientific interpretation of the relief.

The value of a map as a means of representing land forms depends upon two factors: selection of scale and contour interval, and ability on the part of the map maker to express topographic character. The latter prerequisite will not be considered in this discussion. Thus far the factors which have been determinative in the selection of proper scale and contour interval have, as a rule, been: purpose for which the map is made; degree of cultural development of region mapped; cost per square unit; funds available, etc. Definite physiographic criteria have not yet been considered in this connection although the present state of physiographic knowledge is such that the attempt to apply physiographic principles to the mapping of land forms seems opportune and justified. Most topographic maps give little more than an imperfect, incomplete picture of the relief. Others again are overburdened with unnecessary, irrelevant details. Some actually amount to misrepresentations, even though they be the product of sincere and painstaking effort. The topographer is to-day and always has been more or less uncertain as to the matter of detail. Both in the selection of scale and contour interval, and in the actual field sketching he is at a loss to decide which of the smaller topographic units he must show, and which he must leave

out. He needs, in short, criteria to guide him where to draw the line.

The new Yosemite map presents a rather complex problem. Instead of analyzing it and developing the criteria from it, it will be preferable for us to begin by stating the criteria first and then to apply them to the particular case of the Yosemite map.

A topographic map of any kind, because of its small scale, can not undertake to depict the configuration of the land surface complete in all its details. It is essentially in the nature of an abstract, a graphic epitome of the relief. Like any literary abstract, it may be quite brief and confine itself merely to the leading facts; it may be more extended and enter into more or less detail. But whatever its degree of elaborateness, the abridgment of subordinate detail should be evenly maintained throughout, and, above all, its treatment should be *complete so far as it goes.*

The main principle then is that a map must tell a story complete in itself. Its scale and contour interval should be so selected as to admit of the full delineation of every feature essential to the story. No irrelevant subordinate detail should be included if possible.

Some concrete examples may be helpful in illustration.

Let it be required to portray a mountain range in its entirety, with nothing further than its leading characteristics; say a long and narrow block range uplifted along one side, much dissected and enveloped at its base in a broad cloak of waste. These facts may successfully be epitomized by a map on a scale of 1:250,000 with 100 or 200 foot intervals, according to the relief. A smaller scale might leave some of the facts in doubt; a larger scale would introduce superfluous detail.

Again, let it be required to represent the general character of the sculpture of the range. Suppose the range to have been partially glaciated. The new map must be on such a scale as to allow of the distinction between the principal forms of glaciation and those of subaerial erosion. It should be large enough then to admit of the clear delineation of such forms as cirques, arretes, cols, U-canyons, etc., on the one hand, and the characteristic forms of stream and weather erosion on the other. These conditions may be satisfied by a scale of 1:100,000 and 100 or 50 foot intervals. It should be noted that this involves a grouping of the land forms into categories of a new sort. Each group constitutes the record of a certain event in the history of the relief of the range. Together the forms of such a group furnish an index of that event, and collectively they may be conveniently referred to as index forms. The index forms of one event are not necessarily all the product of one and the same process, in fact they seldom are. The index forms of alpine glaciation, for instance, include forms of degradation and of aggradation, and so necessarily do those of subaerial erosion.

If a map then is to tell a complete story, it must aim to show all the essential index forms of one certain event. If it falls short of this it tells the story incompletely; if its scale is such as to admit of subordinate features, the story is unnecessarily encumbered, and the additional cost of mapping is virtually wasted.

Finally, let it be required to make a map of a small portion of the range in question, in order to bring out the local happenings by which a certain feature is differentiated from others of a similar kind. For instance, in the glaciated portion of the range, a certain cirque or canyon may be found possessing decidedly aberrant characteristics. These, being due to local influences, require for their study a map showing the particular index forms in which the incidents due to these local influences may be read. Such a map is the new detail map of the Yosemite Valley (scale 1: 24,000). It aims to represent a glaciated canyon of exceptional form with sufficient detail to shed light on the cause of its aberrant character. Comparison with the standard Yosemite Quadrangle, published several years ago, is interesting in this connection. That sheet, drawn to a scale of 1:125,000, successfully expresses the general character of the sculpturing of the Sierra Nevada. It shows distinctly the glacial sculpture, on the one hand, and the

non-glacial, on the other. It shows the Yosemite Valley together with a host of other glaciated canyons and valleys; but besides giving us an inkling of its unusual nature, tells us nothing except that it has glacial characteristics. The new detail map, on the other hand, depicts the Yosemite Valley, not merely as a glaciated canyon, but as a glaciated canyon in a region of unusual rock structure. It is a map giving index forms of differential erosion and cliff recession, and brings out the fact that the aberrant character of the Yosemite topography is intimately linked with the structural vagaries peculiar to the rocks of the Yosemite region.

*Geology of the Canal Zone:* Ernest Howe.

Passing from the Atlantic to the Pacific, the line of the Panama Canal traverses three well-defined topographic divisions. The first is that of the lower valley of the Chagres and includes the swampy lowlands that extend from Limon Bay nearly to Bohio. This division ends at San Pablo, about six miles below the point where the Rio Obispo enters the Chagres. The second division is that of the summit region and extends from San Pablo to Pedro Miguel, while the third lies between Pedro Miguel and La Boca, and like the first is low and swampy. The relation of these divisions to one another is well shown by the map of the proposed lock canal, the two regions of low relief being marked by artificial lakes while the summit region is traversed by the canal cut.

Although difficult to decipher, on account of the deep covering of red clay and vegetation, the geology is itself quite simple. The oldest rocks of the region are andesitic breccias that occur in the central area between Mamei and Empire and again in the higher hills northeast of Panama. Northward sedimentary rocks occur resting on the older igneous mass and gently inclined toward the Caribbean, so that in passing from the interior toward Colon successively younger beds are encountered. They are well stratified and contain abundant fossils of early Tertiary age. Nearly the same conditions prevail on the Pacific side. On both sides are stratified deposits of acid pyroclastics, the most conspicuous being near the city of Panama. Dikes and large cross-cutting masses of augite-andesite or basalt have invaded all of the older sedimentary rocks, and occur in great abundance in the southern and central parts of the zone; they represent the last phase of active vulcanism in the region.

With the exception of beds of heavy conglomerate in the vicinity of Bohio, all of the sedimentary rocks of the Isthmus consist of argillaceous sandstones, greensands or fine sandy shales; limestones, except high up the Chagres River and in the neighborhood of Empire, are unknown. The rocks are well bedded with moderate northerly dips north of the central region, while the sediments south of the Culebra Cut are inclined in the opposite direction. The oldest beds are Eocene, while the youngest, found near Colon, are late Oligocene. The complete section is preserved only on the northern flanks of the isthmus. Excavation for the locks at Gatun will be in argillaceous sandstones of the early Oligocene throughout, and actual tests on the spot have shown that they are capable of withstanding pressures many times greater than those to which they will be subjected by the lock walls. The earth dam that will be thrown across the valley at Gatun will rest in part upon alluvial material filling a deep gorge cut by the Chagres in Pleistocene time. For more than 100 feet below the surface this alluvium consists very largely of fine blue clay and silt and will be entirely impervious to water. Foundations for the locks at Pedro Miguel will be in the sandy shales and sandstones of the Culebra beds of Eocene age, while the material at the lock site at Sosa at the Pacific end of the canal is massive augite-andesite. The dams at La Boca and Sosa will be of earth and will rest upon alluvial clays of the lower Rio Grande valley.

*Recent Changes in the Ice Fields of Glacier Bay, Alaska:* Charles Will Wright.

Mr. Wright described in some detail the recent remarkable general recession of the glaciers in Glacier Bay, Alaska. A summary of the geologic history of Muir Glacier was

also given and the probable causes of its local advance and recession discussed. In this connection Mr. Wright emphasized particularly the choking and congestion at the valley outlets, as at the mouth of Glacier Bay and locally at Muir Glacier, and the consequent cutting off of warm tidal currents from the ice front. Under such conditions the ice front advanced rapidly, until later on partial removal of the barrier or sinking of the land, the tidal currents regained access to the ice fronts and inaugurated the present period of rapid recession.

Fred E. Wright,
*Secretary*

## DISCUSSION AND CORRESPONDENCE

### DOUBLE-ENDED DRUMSTICKS

To the Editor of Science: The impression was received by more than one person who visited the St. Louis Exposition, that one of the Filipino tribes gathered there used a double-ended drumstick, grasping it in the middle and beating alternately with the ends. Professor O. T. Mason, to whom I applied for light, has most kindly informed me that double-ended drumsticks are occasionally employed to produce variations in sound, the two ends being differently constructed. May I ask if any of the readers of Science can furnish me with the name of a Filipino or other tribe, who handles a drum-beater as above described? I may add that I am especially desirous of knowing of the existence of any photograph showing such a grasp.

H. Newell Wardle

### ARE BULLS EXCITED BY RED?

To the Editor of Science: Is there any real evidence to the effect that bulls are excited by the color red? And how is it with other animals? According to the newspapers, a bull in Sunbury, Pa., charged a window in a millinery store containing an exhibition of red hats and wrecked the store. Is this merely a newspaper myth?

X.

### NOMENCLATURE OF THE CHIRONOMIDÆ

To the Editor of Science: In 1899 Kieffer proposed *Ceratolophus* (*Bull. Soc. Ent. France*, p. 69) as a new genus of Chironomidæ (Midges) with '*femoratus* (Fabr.)' as type. In 1906 the same author reserved this name (Genera Insectorum. Chironomidæ) for a group not containing the type; he also placed '*femorata* Meig.' in two genera at the same time, viz.: *Palpomyia* (p. 63) and *Serromyia* (p. 65). Further, *Ceratolophus* was preoccupied in 1873 (Bocourt, Reptiles).

It is evident that the nomenclature of certain genera of the Chironomidæ is confused and it is a pity that many authors seem to think that thorough unraveling of the nomenclature is unnecessary, when monographing or revising.

G. W. Kirkaldy

## SPECIAL ARTICLES

### SPECIFICATION OF DIAGRAMS IN APPLIED GEOMETRY

By far the greater amount of weariness in reading geometric discussions comes, I think, from the needless labor of searching for and translating the letters describing a figure, into the symbols of the vectors. I have, therefore, been asking myself, whether a few simple rules might not be devised for drawing conventional diagrams, so as to quite eliminate quantities other than those used in the computation. The following plan has assisted me and may be worth remark.

Every vector or arrow is reckoned from a heavy black dot, which I shall call the *but*, to the barb.

When two vectors from the same origin are collinear, the larger vector should step around the barb of the shorter, in the same way in which electrical engineers represent insulated circuits which cross. Conventionally, therefore, a small semicircle, to be called the step-over, is drawn around the arrow point of the shorter vector, *r*, as in Fig. 1.

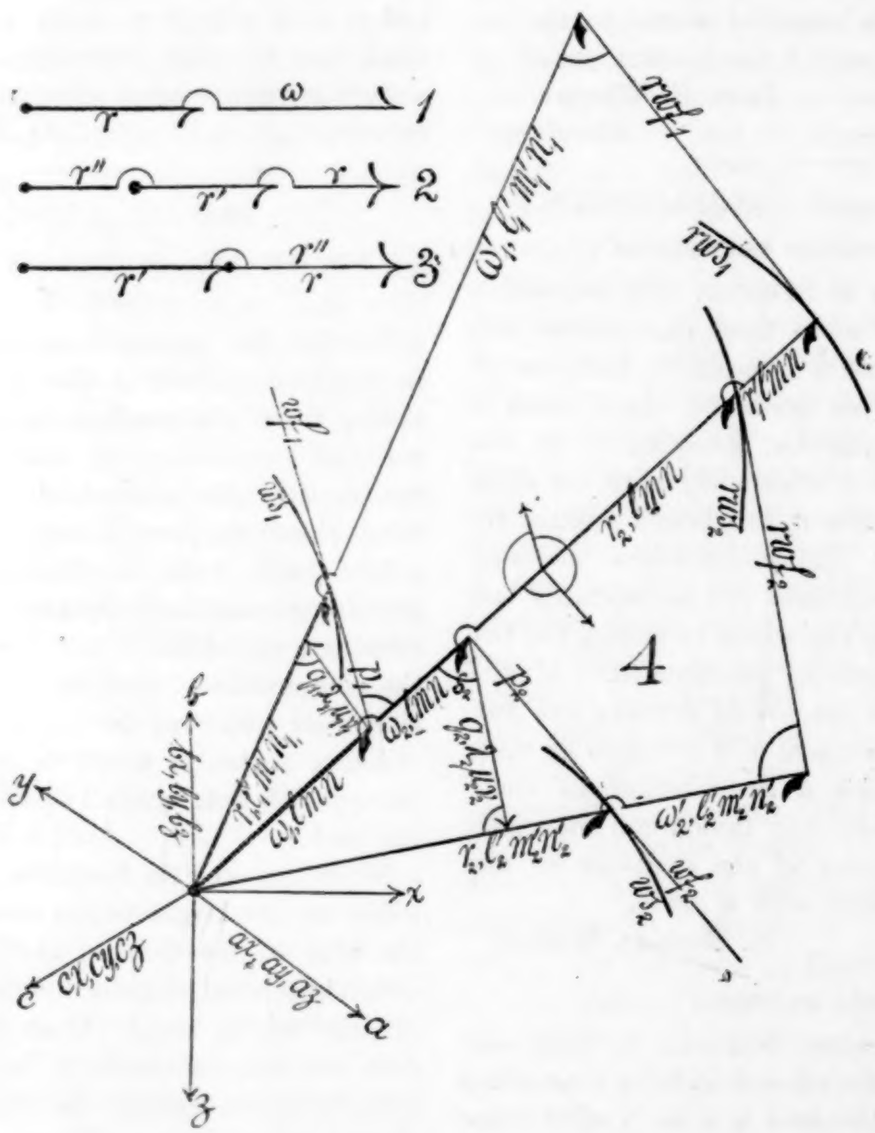
The barb is generally to be drawn *on one side* only, as in the harpoon, and the letter or specification of the vector placed near the barb and (when necessary for clearness) on the same side of the shaft with the barb and step-over. Where several vectors coincide the line may be thickened.

Right angles should be indicated by an arc joining the line. Other angles marked.

When two coincident vectors have not the same origin, both the but and barb of one vector must be stepped over, as in Fig. 2. Each letter refers to the whole vector between the next but and the next barb in order, on either side of it. Thus in Fig. 2, $r''$ and $r$ coincide in $r'$.

When one collinear vector begins where another ends the case is still definite, if the barb and step-over be on the same side of the specification, as stated. Thus in Fig. 3, $r''=r+r'$. Complications, however, should be avoided.

As an example of this method, Fig. 4 shows the case of the Fresnellian wave surface with two nappes, together with the reciprocal wave surface also with two nappes. The figure shows the following qualities without ambiguity.




The axes of coordinates, $x$, $y$, $z$.

The axes of elasticity $a$, $b$, and $c$, each with its three directions.

The two wave normals or velocities $\omega_1$ and $\omega_2$ and their direction cosines, $l$, $m$, $n$.

The two wave fronts, $wf_1$ and $wf_2$.

The two vectors of the wave surface, or the rays $r_1$ and $r_2$, and their direction cosines, $l_1'$, $m_1'$, $n_1'$, and $l_2'$, $m_2'$, $n_2'$.

The two intersection curves of the wave surfaces $ws_1$ and $ws_2$.

The two displacement vectors $p_1$ and $p_2$ and their directions, which are the projections of $r_1$ and $r_2$ on the respective wave fronts $wf_1$ and $wf_2$.

The two force vectors $q_1$ and $q_2$, with their direction cosines, $\lambda_1$, $\mu_1$, $\gamma_1$, and $\lambda_2$, $\mu_2$, $\gamma_2$.

The two intersection curves $rws_1$ and $rws_2$ with the reciprocal wave surface.

The two reciprocal wave fronts $rwf_1$ and $rwf_2$.

The two reciprocal wave normals or reciprocal velocities, $\omega_1'$, $\omega_2'$, and their direction cosines, $l_1'$, $m_1'$, $n_1'$ and $l_2'$, $m_2'$, $n_2'$.

The two vectors $r_1'$ and $r_2'$ of the reciprocal wave surface and their direction cosines, $l$, $m$, $n$.

All the right angles are indicated and the important angle $\delta$, made by the force vector, is shown in one case.

Clearness is given to the diagram by placing the plane normal to the observer's line of sight. The planes $\gamma_1$, $\omega_1$ and $\omega_2$, $\gamma_2$ are at right as the auxiliary vectors show.

Naturally the above method is even more pertinent to the modern methods of vector analysis. The diagrams, like the computations, gain in simplicity. And yet it is just here that authors are peculiarly unwilling to fix the ideas of the student to a definite case. Demonstrations in themselves admirably lucid become confused in effect, because the reader is all the while drifting in the haze of the absolute generality of the statement of the premises.

CARL BARUS

### A PRELIMINARY NOTE ON THE CHROMOSOMES OF ŒNOTHERA LAMARCKIANA AND ONE OF ITS MUTANTS, O. GIGAS

THE exceptional opportunities offered at this station for a study of inheritance as manifested in the germ cells of the *Œnotheras* led me to undertake a study of the chromosomes of *Œnothera Lamarckiana,* its mutants and hybrids.

The work was begun after the flowering season had passed, however; therefore only somatic cells from the growing root tips of potted plants in the rosette stage have so far been available for study; and it is the purpose of this note, pending the completion of a more general study of the *Œnotheras,* merely to call attention to a most unexpected contrast found in the number of chromosomes of *O. Lamarckiana* and one of its mutants, *O. gigas,* both pure bred.

Because of the smallness of the chromatic figures and the low percentage of figures studied in which the chromosomes could be counted with certainty, I do not at present feel justified in stating the exact number in either form; but I can state unreservedly what is of more interest, that in all the somatic cells of *O. gigas* arising from *O. Lamarckiana* in which the chromosomes could be counted with precision, *the number has become approximately double that of the parental form, O. Lamarckiana.* This result was unexpected, as a somewhat hasty survey of the tips of

Oenothera Lamarckiana. Oenothera gigas.

several other mutants previous to the study of *gigas* had indicated a number closely approaching or identical with that of the parental form. Gates, in his "Preliminary Note on Pollen Development in *Œnothera lata* de Vries and its Hybrids," published in SCIENCE, February 15, 1907, states that in a cross resulting from the pollination of *O. lata* by *O. Lamarckiana,* "the sporophyte count for the *O. Lamarckiana* side of the cross is at least twenty. The conclusion from this is that pure *O. Lamarckiana* itself must have over twenty chromosomes." In his paper on "Pollen Development in Hybrids of *Œnothera lata* × *O. Lamarckiana,* and its Relation to Mutation,"[1] he adds in a foot-note on page 109: "The inference that *O. Lamarckiana* itself has the same number of chromosomes as the dominant *O. Lamarckiana* hybrid is also apparently not borne out by the facts." From my own observations on all

[1] *Botanical Gazette,* February, 1907.

pure-bred *O. Lamarckiana* so far studied I have found no indication of the number ever approaching twenty; but from the evidence of repeated counts it seems to be fourteen or fifteen. I have at least eighteen good clear demonstrations of mitotic figures showing only fourteen chromosomes, all distinctly outlined and clearly defined—with no trace of a chromosome in a preceding or following section; on the other hand, I have encountered a sufficient number of less clearly defined figures, in which there seems to be but thirteen, and in others fifteen chromosomes, to make it necessary to state the number for the present with reserve. Chromosomes frequently lie in such positions as to make it impossible to distinguish between a long-looped form and two so placed as to give a similar appearance; also a looped chromosome may be sectioned at a point to give the two halves the appearance of distinct individuals.

The number of chromosomes characteristic of the somatic cells of *O. gigas* is probably twenty-eight or twenty-nine, although the difficulty in counting is here increased by the large number; however, I have six or seven excellent figures showing twenty-eight sharply-defined chromosomes, and as many more, not so clearly outlined, in which there is a strong indication of twenty-nine. It is hoped that the hundreds of new sections now in process of preparation for study will establish the facts, shortly.

Other points of interest are coming to light, particularly in connection with the hybridization of mutants, and will be mentioned in a later note.

Anne M. Lutz
Station for Experimental Evolution,
Cold Spring Harbor, L. I.,
June 28, 1907

## CURRENT NOTES ON LAND FORMS

### DIAMOND HEAD AND MOHOKEA

C. H. Hitchcock has recently described a tuff cone and a caldera in the Hawaiian Islands ("Geology of Diamond Head, Oahu," *Bull. G. S. A.,* XVII., 1906, 469–484; "Mohokea Caldera," *ibid.,* 485–496). Diamond Head, on the island of Oahu, is a well-formed tuff cone with a broad and shallow crater, which the author concludes was thrown up explosively from beneath the level of the sea, the volcanic material having been ejected through fossiliferous limestones of Tertiary age. The cone is compared with the Monte Nuovo near Naples. Considerable attention is given the conflicting theory that the cone was built up gradually by the slow accumulation of material ejected at long intervals. The features of the cone are illustrated by several plates.

Mohokea, on the island of Hawaii, is described as a very irregular caldera only partially enclosed, the open side being toward the sea. Two parallel lines of faulted and tilted lava blocks cross the caldera from southeast to northwest, and are believed to be part of the overlying crust which dropped in when the caldera was formed, although the blocks themselves have been in part crowded up until their crests rise higher than the surface without the caldera. Mohokea is compared with the irregular Haleakala caldera, and illustrations of both are given.

D. W. J.

### A PENEPLAIN IN EQUATORIAL AFRICA

It is generally accepted among physiographers that a peneplain worn down on crystalline rocks in a humid climate would be heavily cloaked with a deep soil of local weathering; and in favor of this opinion the deeply decayed rocks of the somewhat uplifted and dissected Appalachian Piedmont belt may be instanced. In a subarid climate the case is different.

An excellent account of an extensive peneplain, exposing large areas of bare rock, on the southern border of the French Sahara in latitude 18° to 21° N., northeast of the great bend of the Niger and on the arid outer border of the subequatorial (summer) rains, is given by E. F. Gautier ("A travers le Sahara français," *La Géogr.,* XV., 1907, 1–28). The rocks of the region are for the most part Archean granites and gneisses, broken here and there by less ancient igneous intrusions, and associated with belts of strongly folded and

metamorphosed Silurian strata. The Archean area is a smooth platform, a plain of hardly perceptible undulation, its barren surface of bare rock being frightfully desolate; it is interrupted by weathered blocks, often standing in fantastic heaps, and by isolated knobs, which gain an exaggerated appearance of height by reason of the extraordinary flatness of the surrounding plain. The areas of eruptive rocks preserve a greater relief and a more rugged surface; here the trails are at their worst. The Silurian areas possess rounded swells of quartzite between broad shallow depressions; that is, old ridges between old valleys, and thus present a landscape of low, gently modulated forms in contrast with the plain and boulder heaps of the Archean area and with the stronger reliefs of the eruptives. The peneplain as a whole slopes evenly from about 800 m. altitude in the northeast to about 500 m. in the southwest in a distance of 200 kil. It is spoken of as a block, faulted and uplifted in mass. Nevertheless its border on the northeast presents only gentle slopes; there alone are the water courses distinctly enclosed in well-defined valleys beneath the upland surface. Elsewhere the drainage system is highly peculiar, and expresses far-advanced old age, the altitude at which the gently inclined plain now stands being, in the reviewer's opinion, suggestive rather of the inability of the weak, wet-weather, silt-laden streams as yet to have worn their courses closer to normal baselevel, than of elevation in mass after reduction by normal erosion to a lower level. On the Archean area the valleys appear to be so old and the interfluves so completely worn down, that the wadies turn about "sur une pénéplaine rigoureusement horizontale." In the dry season the wadies are not barren stony beds marked by the work of violent floods, like the wadies of the Sahara farther north, but smooth plains of fine alluvium, more or less overgrown with grass and bushes which survive on the ground water stored from the previous wet season.

The alluvium of the wadies is not separated from the bare-rock plains by any distinct border or banks, but the surface of one merges into that of the other; towards the wady border the vegetation thins out and disappears. When the short-lived local rains supply water enough to run from the impermeable rock plains, a wady flows not as a stream, but rather as a sheet of very small depth, great breadth (over a kilometer) and feeble current, soon to be absorbed in the silty alluvium—"une nappe d'épaisseur pelliculaire, très languissament progressive, et bien vite absorbée par l'énorme masse des alluvions." Nowhere else in the world does a single river bear so many names in different parts of its course. A large scale map would show the drainage system of this peculiar region as abnormally, clumsily broadened. Gautier adds pertinent notes on climate, flora, fauna and inhabitants.

W. M. D.

### A PREHISTORIC LANDSLIDE IN THE ALPS

Detrital hillocks in various Alpine valleys, formerly interpreted as moraines, have in more recent years been recognized as prehistoric, usually postglacial, landslides. One of the best examples of the kind is that by Kandersteg, south of Lake Thun, first identified as a landslide by Brückner in 1891; and lately described in detail by V. Turnau (Inaug. Dissert., Univ. Bern, 1906). The material came from a huge nitch, still clearly defined, on the northwestern side of the Fissistock, where the strata dip down the slope and outcrop in basset edges on the steepened wall of the glacially overdeepened valley of the Kander. The nitch is about three kilometers long, and nearly one wide; its upper cliffs reach 3,000 m. altitude; its lower edge lies at 1,500 m. The detritus occupies the Kander valley for a length of 8 kil., northward from the point of its oblique entrance, with a width of from a half to one kilometer, the valley floor at the entrance of the slide being 1,200 m., and at its lower end, 800 m. altitude. The highest part of the slide is opposite its source, where the gliding mass was banked up against the opposite valley wall. The thickness of the detritus is seen to vary from 150 to 30 m., but its bottom is not observed. Its form is extremely irregular, and it has greatly

obstructed the flow of the valley stream. Its surface is strewn with large blocks of rock. Its volume is estimated at 900,000,000 cu. m. A little farther east, in a branch of the Kander valley, a similar but much smaller slide forms the barrier by which Oeschinen lake is enclosed in a cirque-like valley head.

Turnau quotes estimates of the volumes of other landslides. That of Elm, which happened a score of years ago, is 10,000,000 cu. m.; that of Goldau, a century ago, 15,000,000. Far greater was the prehistoric landslide of Flims in the upper valley of the Rhine, which is estimated at 15,000,000,000 cu. m.; or a thousand times greater than the Goldau slide, and even sixteen times greater than the great slide of the Kander valley.

W. M. D.

## *PRELIMINARY LIST OF SCIENTIFIC COMMUNICATIONS TO BE PRESENTED AT THE SEVENTH INTERNATIONAL ZOOLOGICAL CONGRESS, BOSTON, AUGUST 19 TO 23, 1907*

In response to the invitations of the General Committee and of the various secretaries of the organization a generous number of acceptances to address the congress or to read papers before its sections have been received. The communications thus submitted fall under three heads: addresses, for which the speakers have been invited; voluntary communications to be given before sections; and demonstrations. The number and quality of these contributions presage an unusually successful meeting.

The following speakers have consented to deliver addresses either before the general meetings or the sectional meetings: Professor W. Bateson, Cambridge, England; Professor C. Depéret, Lyons, France; Dr. H. Driesch, Heidelberg, Germany; Dr. T. N. Gill, Washington, D. C.; Professor Richard Hertwig, Munich, Germany; Dr. G. Horváth, Budapest, Hungary; Dr. L. O. Howard, Washington, D. C.; Professor A. A. W. Hubrecht, Utrecht, Holland; Professor J. Loeb, Berkeley, Cal.; Professor C. E. McClung, Lawrence, Kan.; Professor J. P. McMurrich, Ann Arbor, Mich.; Sir John Murray, Edinburgh, Scotland; Dr. R. F. Scharff, Dublin, Ireland; and Professor C. O. Whitman, Chicago, Ill.

For the presentation and discussion of scientific communications fifteen sections have been tentatively named and the organization of each section has been put into the hands of a secretary to whom requests concerning that section should be addressed. The names of the sections and the persons having charge of them are as follows: General Zoology, F. R. Lillie, Chicago, Ill.; Systematic Zoology, D. S. Jordan, Stanford University, Cal.; Entomology, L. O. Howard, Washington, D. C.; Ornithology, Witmer Stone, Philadelphia, Pa.; Palæozoology, H. F. Osborn, New York, N. Y.; Comparative Anatomy, C. S. Minot, Boston, Mass., and J. S. Kingsley, Tufts College, Mass.; Embryology, E. G. Conklin, Philadelphia, Pa.; Cytology, E. B. Wilson, New York, N. Y.; Zoogeography, L. Stejneger, Washington, D. C.; Thalassography, W. E. Ritter, Berkeley, Cal.; Applied Zoology, C. W. Stiles, Washington, D. C.; Comparative Physiology, W. B. Cannon, Boston, Mass.; Experimental Zoology, T. H. Morgan, New York, N. Y.; Heredity, C. B. Davenport, Cold Spring Harbor, N. Y., and Animal Behavior, H. S. Jennings, Baltimore, Md.

The following preliminary list of communications is announced for the sectional meetings:

T. B. Aldrich: Title not yet received.

R. J. Anderson: "Notes on the Movements in Some Animals, with especial reference to their Susceptibility to Training," "A Short Review of the Mammalian Mandible," "Illustrations Suggestive of the Mode of Formation of the Cetacean Flipper."

S. von Apáthy: "New Method of Making Serial Celloidin Sections," "An Unintentional Experiment on Living Nuclei and the Real Structure of the Cell Nucleus," "New Facts and Critical Notes about Neurofibrillæ," "The Presence of Krause's Membrane as a General Feature of Striated Muscles."

S. Awerinzew: "Ueber die Myxosporidien von *Drepanopsetta platessoides*," "Die Marine Biologische Station an der Murman-Küste."

C. R. Bardeen: Title not yet received.

P. Bartsch: "A Study in Distribution based

on the Family Pyramidellidæ of the West Coast of America."

W. BATESON: Title not yet received.

C. W. BEEBE: "Geographic Variation in Birds, with special reference to Humidity."

R. R. BENSLEY: "An Analysis of the Salivary Glands of Mammals based on Cellular Characters."

E. A. BIRGE: Title not yet received.

R. BLANCHARD: Title not yet received.

MARY BLOUNT: "On the Cleavage and Formation of the Periblast and the Germ Wall in Pigeons."

KRISTINE BONNEVIE: Title not yet received.

G. M. BOWERS: "Work of the *Albatross*."

K. BRANDT: Title not yet received.

W. S. BRUCE: "Life of the Sea in the Antarctic Regions."

H. L. BRUNER: "Cephalic Brains of Reptiles."

E. BUGNION: "Fasceux spermatiques doubles (bipolaires) des Ténétrions et des Milabres de Ceylon et d'Egypte."

G. N. CALKINS: "Origin and Division of the Maturation Chromosomes in *Paramœcium*."

R. E. CALL: "Revision of the North American Unionidæ."

W. B. CANNON: "The Acid Control of the Pylorus."

W. B. CANNON and H. H. SMITH: "Some Observations on the Action of the Cardia with reference to a Probable General Law of Motor Activity in the Alimentary Canal."

A. J. CARLSON: "On Salivary Secretions."

W. E. CASTLE: "Reversion and the Fixation of Characters," "The Mendelian Inheritance of Sex."

FRANK M. CHAPMAN: "Remarks on the Geographical Origin of North American Birds." (With slides.)

C. M. CHILD: "Amitosis and Mitosis in Normal and Regular Growth."

T. D. A. COCKERELL: "The Miocene Fauna of Florissant, Colorado."

W. R. COE: Title not yet received.

L. W. COLE: "Behavior of Raccoons under Experimental Conditions."

C. CORRENS: Title not yet received.

E. R. CORSON: "Fusion of Cuneiform and Semilunar Bones."

R. P. COWLES: Title not yet received.

W. C. CURTIS: Title not yet received.

W. H. DALL: "Deep-water Distribution of the Molluskan Fauna of the Northwest Coast."

W. DANTCHAKOFF: "Ueber die Entwicklung der Blutelemente beim Huehnerembryo."

A. D. DARBISHIRE: "Mendelian and Sexual Inheritance."

H. B. DAVIS: "The Behavior of Raccoons."

A. DAVISON: "Modifications of Structure in a Dog born with Three Legs."

J. DAWSON: "The Behavior of Physa."

B. DEAN: "'Precocious Segregation' as shown in Series of Vertebrate Embryos and its Significance in Mosaic Development."

K. DERJUGIN: "Murmansche Biologische Station der K. Naturforscher Gesellschaft St. Petersburg und ihre Untersuchungen im nordischen Eismeere."

H. H. DONALDSON: "The Nervous System of *Rana virescens* compared with that of *Rana temporaria* and *R. esculenta*."

HANS DRIESCH: "The Stimuli of Restitutions."

J. E. DUERDEN: "The Influence of Domestication on the Behavior of the Ostrich."

H. G. DYAR: "The Distribution of Mosquitoes in North America."

C. L. EDWARDS: "Variations in the Holothurioidea."

C. H. EIGENMANN: "The Ecology of the Cuban Blind Fishes," "Origin and Distribution of South American Fresh-water Fishes."

B. W. EVERMANN: "The Origin of the Golden Trout of the Southern High Sierra."

E. FAWCETT: "The Development and Ossification of Certain Bones in the Human Body."

P. A. FISH: "The Exchange of Air in the Guttural Pouches of the Horse."

O. FOLIN: "On Protein Metabolism in Fasting."

S. A. FORBES: "A Statistical Study of the Local Distribution and Ecology of Birds."

A. FRORIEP: "Ueber die Cranio-vertebralgrenze bei den Amphibien, im Vergleich zu Selachier und Amnioten."

S. H. GAGE: "Glycogen in the Embryos of *Petromyzon*, *Amblystoma* and *Gallus*," "Glycogen in the Embryo."

S. P. GAGE: "A Four-weeks Human Embryo," "Method of Making Models of Blotting-paper instead of Wax," "Brain of a Six-weeks Human Embryo."

A. GAUDRY: Title not yet received.

T. N. GILL: "The Incongruity of Inland and Marine Fauna."

O. C. GLASER: "A Rapid Method of Demonstrating Habit Formation."

J. GOLDBERGER: Title not yet received.

L. VON GRAFF: "Zur Anatomie der nordamerikanischen Turbellarien," "Vergleichung der nordamerikanischen und europaischen Turbellanienfauna."

C. GRAVE: Title not yet received.

C. C. Guthrie: "Further Results of Transplantation of Ovaries in Chickens."

M. F. Guyer: "Deficiencies of the Chromosome Theory of Heredity," "Results of Injecting Blood and Lymph into Unfertilized Frog Eggs."

P. B. Hadley: "The Behavior of Young Stages of the Lobster."

G. V. Hamilton: "An Experimental Study of an Unusual Type of Reaction in a Dog."

C. W. Hargitt: "The Behavior of Tubicolous Animals," "The Organization and Early Development of Cœlenterate Eggs."

M. Hartog: "Rheotaxy of Copepods and Rotifers."

Y. Henderson: "A Graphic Expression of the Principles Underlying the Normal Variations in the Behavior of the Mammalian Heart."

H. W. Henshaw: "The Zoogeographic Work of the Biological Survey."

W. A. Herdman: Title not yet received.

W. B. Herms: "The Reactions of Sarcophagid Fly Larvæ to Light."

A. L. Herrera: "The Comparative Behavior of Insects and of Winged Seeds," "Plasmogeny, a New Experimental Science."

F. H. Herrick: "Organization of the Gull Community: a Study of the Communal Life of Birds."

M. Herzog: "On the Earliest Stage of Placentation and on the Earliest Development of the Embryo in Man."

L. T. Hobhouse: "The Importance of Animal Psychology for the Theory of Evolution."

G. Horváth: "Relations entre les faunes hémiptérologiques de l'Europe et de l'Amérique du Nord."

L. O. Howard: Title not yet received.

W. E. Hoyle: "The Cephalopoda of the *Albatross* Expedition, 1904–5."

G. S. Huntington and C. F. W. McClure: "The Early Stages in the Development of the Mammalian Lymphatic System and their Correlation to the Embryonic Venous Channels," "Anatomy and Development of the Lymph-sac and Thoracic Duct in the Domestic Cat."

C. C. Hurst: "Unit Characters in Animals and Plants."

H. S. Jennings: Title not yet received.

R. H. Johnson: "Heredity of Color Pattern in Coccinellid Beetles."

J. B. Johnston: "The Phylogenetic History of the Somatic Sensory Column of the Vertebrate Brain."

W. E. Kellicott: "The Degree of Correlation as a Selective Basis."

V. L. Kellogg: "Heredity in Silkworms."

J. S. Kingsley: "A Neglected Point in the Quadrate Problem."

P. Kyes: "The Lecithin Content of the Red-blood Corpuscles and its Relation to the Stroma."

A. Lang: "Hybrids of *H. nemoralis* and *H. hortensis*."

F. S. Lee: "The Nature and Cause of Muscle Fatigue."

F. T. Lewis: Title not yet received.

E. Linton: "Notes on the Distribution of Entozoa of North American Marine Fishes."

W. A. Locy: "The Fifth and Sixth Aortic Arches in Birds and Mammals," "Injected and Dissected Chick Embryos showing Fifth and Sixth Aortic Arches," "The Nervus Terminalis in Selachians."

A. M. Lutz: "A Study of the Chromosomes of *Œnothera Lamarckiana:* its Mutants and Hybrids."

F. E. Lutz: "Inheritance of Abnormal Wing Venation in *Drosophila*."

M. W. Lyon, Jr.: "The Distribution of Bats in the Zoogeographical Regions."

O. Maas: "Kaltenziehung und Hunger bei Spongien."

J. P. McMurrich: "Intermembral Homologies."

C. L. Marlatt: Title not yet received.

S. O. Mast: "Light Reaction in *Volvox*."

W. D. Matthew: Title not yet received.

A. G. Mayer: "The Annual Swarming of the Atlantic Palolo," "A System of the Hydromedusæ," "Vantage Grounds for the Study of the Marine Life of the West Indian Region," "Factors Controlling Rhythmical Pulsation."

A. Maximow: "Ueber die Entwicklung der Blutelemente beim Saeugetierembryo."

W. J. Meek: "On the Selective Action of Certain Drugs."

S. Metalnikoff: "Sur l'immunité de la Galleria mellonella vis à vis des cacilles tuberculeux."

A. Meyer: "The Homologies of the Mesial Wall of the Cerebral Hemisphere of Vertebrates."

C. S. Minot: "Changes in the Nuclei of Vertebrates in Relation to Age."

T. H. Montgomery, Jr.: "Fertility of Egg of *Theridium*," "The Maturation and Fertilization of the Spider's Egg."

A. Mrázek: Title not yet received.

J. P. Munson: "The Anatomy of *Ophioglypha Sarsii*," "Observations on the Generation and Degeneration of Sex-cells."

H. F. Nachtrieb: "Lateral Line System of *Polyodon spathula*."

H. V. Neal: "The Development of the Ventral Cranial Nerves in *Squalus*."

H. W. NORRIS: "The Cranial Nerve Components of *Amphiuma*."

J. P. NUEL: "The Reactions of Flatworms to Light."

C. C. NUTTING: "The Color of Deep-sea Animals Considered in Connection with Phosphorescence."

A. E. ORTMANN: "The Necessity of Survey Work in Zoographical Research," "The Double Origin of Marine Polar Faunas," "Variations and Changes of Environment."

H. F. OSBORN: Title not yet received.

W. PATTEN: "On the Origin of Vertebrates."

S. PATER: "Some of the Earliest Reactions of the Vertebrate Embryo and their Relations to the Nervous System."

J. T. PATTERSON: "On Gastrulation in Birds."

P. PELSENEER: "Bipolar Theory," "Considérations sur la genèse du névraxe, spécialement sur celle observée chez le Pélobate brun."

A. W. PETERS: "The Action of Pure Water on Living Cells," "The Function of the Inorganic Salts of the Protozoan Cell and its Medium."

A. PETRUNKEVITCH: "The Sense of Sight in Spiders."

J. P. PORTER: "The Behavior of Birds."

H. PRZIBRAM: Title not yet received.

F. H. PRATT: "Vasomotor Reflexes to Isolated Vascular Areas."

H. E. RADASCH: "Unilateral Absence of Genito-Urinary System and its Relation to the Development of Wolffian and Muellerian Ducts."

B. H. RANSOM: Title not yet received.

A. M. REESE: "Embryology of the Florida Alligator."

W. E. RITTER: "A Point of View with Reference to the Problem of Organic Evolution," "The Problem of Adaptation to Conditions of Life at Great Depths, as exemplified by Simple Ascidians."

L. RHUMBLER: "Zellenmechanik und Vererbung."

L. ROULE: "L'Origine de la notochorde et du neuraxe chez les larves urodeles des Tuniciers."

C. F. ROUSSELET: "Improvements in the Method of Preservation of Rotatoria."

W. ROUX: Title not yet received.

R. F. SCHARFF: "On the Evolution of Continents as illustrated by the Geographical Distribution of Existing Animals."

V. E. SHELFORD: "Behavior of the Tiger Beetles: Its Bearing on Variation and Distribution."

A. E. SHIPLEY: Title not yet received.

C. H. SHULL: "Results of Hybridizing *Bursa pastoris* and *Bursa heegeri*."

W. J. SINCLAIR: "The Santa Cruz Typotheria."

H. M. SMITH: Title not yet received.

J. B. SMITH: "Ridding a State of Mosquitoes."

A. STEUER: "Seasickness."

N. M. STEVENS: "Various Types of Heterochromosomes in the Coleoptera," "The Chromosomes in *Drosophila ampelophila*."

C. W. STILES: Title not yet received.

F. B. SUMNER: "The Effects upon Fishes of Changes in the Chemical or Physical Properties of their Surrounding Medium."

O. P. TERRY and E. P. LYON: "Further Observations on Ferment Activity in Unfertilized and Fertilized Eggs."

J. A. THOMSON: "A New Type of Alcyonarian," "As Regards Germinal Selection."

E. L. THORNDIKE: Title not yet received.

W. L. TOWER: "Experimental Evidence Concerning the Existence of 'Unit Characters' and the Relation of this Evidence to the Theory of Mutation," "Experimental Production of Progressive Evolution without Saltation," "Continuity *vs.* Discontinuity in Animal Evolution."

C. H. TURNER: "Do Ants form 'Practical Judgments'?"

J. W. VAN WIJHE: "The Chondrocranium in Birds."

T. W. VAUGHAN: "Results of a Study of Recent Madreporaria of the Hawaiian Islands and Laysan."

F. VEJDOVSKY: "Giebt es eine Reduktionsteilung?" "Ueber den Ursprung der Lymphocyten."

H. B. WARD: Title not yet received.

J. WARREN: "Epiphysial Region in Reptilia and *Necturus*," "The Paraphysis and Pineal Region in *Lacerta*," "The Paraphysis and Pineal Region in *Chrysemys marginata*."

A. S. WARTHIN: "Regional Hæmolymph Nodes, a Comparative Study."

E. WASMANN: Title not yet received.

W. M. WHEELER: "Social Insects and the Inheritance of Somatogenic Characters," "The Origin of Slavery among Ants."

B. G. WILDER: "Certain Simplified Terms, with Reasons for their Adoption."

A. WILLEY: "Lecithality, Oviposition and Viviparity."

E. B. WILSON: "Illustrations of the Morphological and Physiological Individuality of the Chromosomes in the Hemiptera."

N. YATSU: "An Experimental Study on the Cleavage of the Ctenophore Egg."

R. M. YERKES: "Behavior of the Dancing Mouse."

E. YUNG: "Le sens de l'humide chez les Mollusques," "Structure des tentacules chez *Helix*."

C. ZELENY: Title not yet received.

Notices have also been received that the following demonstrations will be made:

S. VON APÁTHY: "Certain Instruments for Microtechnique," "Microscopic Preparations."

C. W. BEEBE: "Bird Skins."

W. DANTCHAKOFF: "Microscopic Preparations."

G. A. DREW: "Illustrations of a Method to make Series of Anatomical Drawings."

J. H. EMERTON: "Demonstration Collection of Spiders."

W. JUNK: "Zoological Books."

F. E. LUTZ: "Abnormal Wings of *Drosophila*."

O. MAAS: "Microscopic Preparations."

A. MAXIMOW: "Microscopic Preparations."

S. METALNIKOFF: "Preparations of Blood and of Bacillus tuberculosis."

A. PETRUNKEVITCH: "Images in the Spider's Eyes."

C. F. ROUSSELET: "Mounted Slides of Rotifera."

J. A. THOMSON: "Peculiar Alcyonarians."

Members are reminded that notices of communications, demonstrations, etc., should now be in the hands of the General Committee of the Seventh International Zoological Congress, Cambridge, Mass.

For the Executive Committee,
G. H. PARKER, *Chairman*

## RADIUM EMANATION[1]

IN 1903, it was shown by Mr. Soddy and myself that the spontaneous change of the emanation from radium results in the formation of helium; this observation has been confirmed by Indrikson, by Debierne, by Giesel, by Curie and Dewar, and by Himstedt and G. Meyer. Debierne has shown that actinium chloride and fluoride also develop helium. I have also once detected helium in the gases evolved continuously from a solution of thorium nitrate, and hope soon to confirm this observation.

When the emanation is in contact with, and dissolved in water, the inert gas which is produced by its change consists mainly of neon; only a trace of helium could be detected.

[1] From *Nature*, July 18. This letter is apparently the basis of the alleged interview with Sir William Ramsay, cabled to a prominent New York newspaper on July 28 and widely quoted.

When a saturated solution of copper sulphate is substituted for water, no helium is produced; the main product is argon, possibly containing a trace of neon, for some of the stronger of its lines appeared to be present. The residue, after removal of the copper from this solution, showed the spectra of sodium and of calcium; the red lithium line was also observed, but was very faint. This last observation has been made four times, in two cases with copper sulphate, and in two with copper nitrate; all possible precautions were taken; and similar residues from lead nitrate and from water gave no indication of the presence of lithium; nor was lithium detected in a solution of copper nitrate, similarly treated in every respect except in its not having been in contact with emanation.

These remarkable results appear to indicate the following line of thought: From its inactivity it is probable that radium emanation belongs to the helium series of elements. During its spontaneous change, it parts with a relatively enormous amount of energy. The direction in which that energy is expended may be modified by circumstances. If the emanation is alone, or in contact with hydrogen and oxygen gases, a portion is "decomposed" or "disintegrated" by the energy given off by the rest. The gaseous substance produced is in this case helium. If, however, the distribution of the energy is modified by the presence of water, that portion of the emanation which is "decomposed" yields neon; if in presence of copper sulphate, argon. Similarly the copper, acted upon by the emanation, is "degraded" to the first member of its group, namely, lithium; it is impossible to prove that sodium or potassium are formed, seeing that they are constituents of the glass vessel in which the solution is contained; but from analogy with the "decomposition-products" of the emanation, they may also be products of the "degradation" of copper.

A full account of this research will shortly be communicated to the Chemical Society.

WILLIAM RAMSAY.

July 11.

## SCIENTIFIC NOTES AND NEWS

Sir Henry Roscoe has been elected a foreign member of the Accademia dei Lincei in Rome.

In connection with the celebration of the centenary of the Geological Society, London, Oxford University will, on September 30, confer the degree of doctor of science on Professor Charles Barrois, Lille; Professor A. Heim, Zürich; Professor A. Lacroix, Paris; Professor A. Penck, Berlin; Dr. Hans H. Reusch, Norway; Professor F. Zirkel, Leipzig.

The University of Liverpool has conferred its doctorate of science on Professor A. R. Forsyth, of Cambridge; Professor Francis Gotch and Professor Osler, of Oxford; Sir Oliver Lodge; Sir John Murray, the naturalist; Professor Wilhelm Ostwald, of Leipzig; Professor Sir William Ramsay; and Sir H. E. Roscoe; also Dr. C. L. A. Laveran, of the Pasteur Institute (*in absentia*). The degree of doctor of engineering has been conferred on Sir A. B. W. Kennedy.

Professor Anders Donner, director of the Royal Astronomical Observatory, Helsingfors, Finland, has been elected an Associate of the Royal Astronomical Society.

On the occasion of the last lecture of Professor G. Lunge, whose retirement from the chair of technical chemistry at the Zürich Polytechnic we have noticed, addresses from the teaching staff and from the students were presented by Professor Treadwell.

Professor W. J. van Bebber has retired from his position as departmental head in the Deutsche Seewarte at Hamburg.

The Mississippi Valley Laboratory of the United States Department of Agriculture has been abolished, and the work in forest pathology will from this time be carried on at Washington, D. C. The Laboratory of Forest Pathology, as now organized, is under the charge of Dr. Haven Metcalf. With him are associated Drs. George G. Hedgcock and Perley Spaulding, who were formerly at the Mississippi Valley Laboratory.

The Alvarenga prize of the College of Physicians of Philadelphia, for 1907, has been awarded to William Louis Chapman, M.D., Providence, R. I., for investigations on "Postoperative Phlebitis, Thrombosis and Embolism."

The Mackinnon studentship of the Royal Society in physical science has been awarded for a second year to Mr. W. Geoffrey Duffield, for a research on the influence of pressure on spectra, being conducted at the University of Manchester; and the studentship in biology to Dr. H. M. Woodcock, to aid him in working out the life history of certain hematozoa of birds, an investigation which will be carried on at the Lister Institute of Preventive Medicine. The income of the Gunning fund accrued during the past three years has been placed at the disposal of Dr. F. H. Scott for the continuation of his investigations into the metabolic processes in nerve cells.

Dr. Reid Hunt, of the U. S. Public Health and Marine Hospital Service, will take part in a discussion on the physiological and therapeutic uses of alcohol before the section of physiology of the British Association, now meeting in Leicester.

The British Medical Association is holding its seventy-fifth annual meeting this week at Exeter, under the presidency of Dr. Henry Davy. The address in medicine will be delivered by Dr. William Hale White, the address in surgery by Dr. Henry Trentham Butlin and a popular lecture by Sir John William Moore.

The tercentenary of the death of Ulisse Aldrovandi, the celebrated naturalist, was celebrated at Bologna, from June 11 to 13, in the presence of numerous delegates from foreign countries. A memorial tablet was unveiled, while a medal and several volumes compiled for the occasion were presented to the delegates.

Professor Angelo Heilprin, the eminent naturalist and explorer, died on July 17. He was born in Hungary in 1853 and was brought to this country when he was three years old. Professor Heilprin has been

since 1881 professor of paleontology and geology in the Philadelphia Academy of Natural Sciences and had recently been appointed lecturer on physical geography at Yale University.

Dr. August Dupré, F.R.S., chemical adviser to the explosive department of the Home Office of the British government, and the author of papers on chemical and other scientific subjects, died on July 16. He was born in Maintz in 1835, and went to London in 1855.

Admiral John Maclear, of the British navy, commander of the *Challenger* on its scientific voyage from 1872–6 and captain of the *Alert,* and the *Flying Fish,* with which much important hydrographic work was done, died at Niagara Falls on July 17, at the age of sixty-nine years. He was the son of Sir T. Maclear, formerly astronomer royal at the Cape of Good Hope. His wife was a daughter of Sir John Herschel.

Professor Heinrich Kreutz, associate professor of astronomy at Kiel and editor of the *Astronomische Nachrichten,* died on July 16.

Professor J. J. Grancher, an eminent French physician, known especially for his efforts to combat tuberculosis, has died at the age of sixty-four years.

M. André Prosper Paul Crova, professor of physics at Montpellier and known for his researches on radiation and other subjects, has died at the age of seventy-four years.

The French government has recommended a grant of $60,000 for a French expedition to the antarctic regions.

By the will of the late Mr. Mark Stirrup Manchester University has received specimens of volcanic rocks and fossils; £1,000 for the maintenance of a geological and paleontological collection and £1,500 for the foundation of a paleontological scholarship.

A telegram has been received at Harvard College Observatory from Professor Percival Lowell, director of the Lowell Observatory, stating "Martian double canal Gihon photographed double by Lampland and also by me."

*Nature* states that a small exhibition of scientific apparatus, mostly for chemistry and physics, is being arranged by Mr. R. E. Thwaites, of Wyggeston Grammar School, in connection with the meeting of the British Association, which opened this week at Leicester.

## UNIVERSITY AND EDUCATIONAL NEWS

Dr. Geo. L. Streeter, associate professor of neurology at the Wistar Institute of Anatomy, Philadelphia, has been elected professor of anatomy at the University of Michigan.

Dr. J. Heath Bawden, of Vassar College, has accepted the professorship of philosophy at the University of Cincinnati.

Dr. R. P. Stephens, instructor in mathematics in Wesleyan University, has been elected adjunct professor of mathematics in the University of Georgia. Mr. Berton H. Camp has been appointed instructor in mathematics in Wesleyan University.

Appointments at Syracuse University have been made as follows: Joseph E. Kirkwood, professor of botany; W. M. Smallwood, professor of compaiative anatomy; Charles G. Rogers, associate professor of physiology; C. H. Richardson, associate professor of geology and mineralogy; Daniel Pratt, assistant professor of mathematics; Herbert A. Clark, assistant professor of physics; Howard F. Hart, instructor in mathematics; Roger F. Brinell, instructor in chemistry; Burnett Smith, instructor in geology; George D. Babcock, professor of practical mechanics; Forrest E. Cardullo, associate professor of machine design; Carl H. Beach, assistant professor of machine design; James B. Faulks, associate professor of experimental engineering; Dr. Harold D. Senior, professor of anatomy and director of the anatomical laboratory; Dr. L. D. Bristol, instructor in pathology and bacteriology.

Mr. J. W. Bews has been appointed to a newly-established lectureship of economic botany at Manchester.

Dr. F. R. Noll, of the Agricultural Academy at Poppelsdorff, has been made professor of botany at Halle, and Dr. Gerhard Hassenberg acting professor at Poppelsdorf.